the halls, struck the keeper, making his nose bleed. You were there." The illusion soon developed; and the subject repeated the whole story, adding that a nurse came with a basin of water to wash off the blood. A neighboring subject was then aroused, and asked what happened yesterday to No. 3. After some hesitation, he repeated the story. And so on with all the others, including one who was sleeping naturally. No. 3 himself admitted that he struck the keeper, but he did not begin the quarrel. None of these patients had ever assisted at such an experiment before. The experiment may not succeed at all times and with all subjects; but it shows, that, when the sleeper has his attention fixed upon the person who is speaking, he hears and accepts every thing. On awakening, he does not recall this of his own accord; but, as soon as a hint is given, he recalls it all, and accepts it as a reality. As a practical outcome of the observation, Dr. Bernheim gives the warning not to tell secrets in the presence of a sleeper.

Statistics of Cures by Hypnotism.

The methods and purposes of the clinic for the treatment of diseases by hypnotism, founded at Amsterdam by Drs. van Reuterghem and van Eeden, have been noticed before in these columns (*Science*, May 24, 1889). On the occasion of completing the first two years of their experience, they have put together an account of the kind and number of diseases treated, and the amount of success achieved; and these statistics, being comparatively extensive and carefully collected, have good claims to general consideration. There were treated, in all, 414 patients (219 men and 195 women). Of these, only 15 (less than 4 per cent) could not be hypnotized; 217 (53 per cent) entered a light stage of sleep; 135 (32 per cent) entered a deeper stage; and 47 (11 per cent) entered the somnambulic stage, characteristic of the best hypnotic subjects. The ages of the patients were distributed as follows: from 1 to 10 years, 9; from 11 to 20 years, 46; from 21 to 40 years, 203; from 41 to 60 years, 131; from 61 to 80 years, 25. There were 361 of the 414 afflicted with various kinds of nervous troubles, 168 were classed as general neuropathic disorders, 68 as neuralgias and pains, 60 as mental diseases, 40 as hysterical affections, and 29 as organic affections. In general, the effects of the treatment are indicated by the following figures: no effect in 71 cases (20 per cent), a slight or passing improvement in 92 cases (26 per cent), a distinct and permanent improvement in 98 cases (27 per cent), and a cure in 100 cases (28 per cent). The disproportion in the number of nervous and non-nervous cases makes a fair comparison of the results in the two classes impossible. Among the nervous diseases, those classed as neuropathic show a very favorable result, 33 per cent being cured, and 26 per cent permanently benefited. Hysterical and neuralgic affections show nearly as high a percentage, though the absolute numbers are here much smaller. Diseases classed as organic naturally show the very minimum of success in treatment. We have thus no announcement of hypnotism as a panacea curing all diseases, but a fair proportion of success and failure distributed among various disorders in a way that accords with our knowledge of the nature of such diseases. It is only by such impartial and scientifically collected results that the movement can make progress.

AMONG THE PUBLISHERS.

LAST week's issue of *Garden and Forest* contains an excellent illustration of the famous Waverly Oaks, near Boston, and a figure of *Gladiolus turicensis*, one of the noteworthy additions to garden-plants last year. Mr. Charles Eliot writes instructively of the coast of Maine; and among other contributors to the number are Professor J. B. Smith, Professor W. A. Buckhout, Professor E. S. Goff, Professor J. T. Rothrock, Dr. Udo Dammer, John Thorpe, and Mrs. Schuyler Van Rensselaer.

— The March number of the *New England Magazine* will contain many portraits. In the article on the "Supreme Court of the United States" there will be given likenesses of more than a dozen of the great justices. In an article on "Chautauqua" will be found portraits of Bishop Vincent and Mr. Lewis Miller. "A Successful Woman's Club," "A Strange Dinner-Party," and "An Old New England Country Gentleman," are other illustrated articles in this number.

— To meet the demand for a much greater variety and number of illustrations in the *American Architect*, Messrs. Ticknor & Co. have arranged to more than double the extent of that department, and to add many new features. To give their subscribers a greater amount of illustration, it is necessary to increase the subscription price, but only to those who desire the increased illustration. They therefore continue their regular and imperial editions, but have issued, in addition, an enlarged and more expensive edition, called "the international edition." The international includes all that the imperial contains (that is, the equivalent of 384 pages of photo-lithographic illustration of all sorts, also 40 gelatine and 12 heliochrome plates, and the extra photogravure plate for the year), and adds (A) a large amount of foreign work, received regularly from England, France, and Germany. The apportionment of this new matter is not yet finally settled, but it will amount approximately to over 200 pages of photo-lithographs, and probably 150 gelatine plates, besides a large number of genuine copperplate etchings. To give still further value to this edition, there will be from time to time (B) additional colored prints and (C) real photogravures, — genuine copperplate prints, such as are issued by Messrs. Goupil in Paris by that name. But the feature perhaps the most interesting to the American profession will consist (D) in publishing in this international edition, as far as subscribers will aid, competitive designs submitted in limited, and in some cases in public, competitions. To do this — to provide a journal containing approximately 1,000 page illustrations (besides nearly as many smaller cuts in the text) and (E) an attendant increase in the text of four pages weekly, 200 pages per annum — has required a considerable increase in the subscription price, and it cannot be placed at less than $25 per annum. At the same time, to place it within reach of many to whom so large a single payment might be an inconvenience, quarterly payments at a slightly increased rate may be made when preferred. No subscriptions will be received, however, for less than the full calendar year, as the plans involve contracts in at least three foreign countries, made upon a permanent basis by the year. There has just been issued in the *American Architect* a photogravure from Mr. Axel H. Haig's famous etching, "At the Fountain of St. George." This is commonly called "St. George at Lubeck;" but Mr. Haig writes, "The subject is not to be found at Lubeck at all or in any North German town. The work is a composition, partially founded on a scene in an old Bavarian town, but, being so very much an invention, I cannot give a locality to it."

— "The danger of an ignorant person in seizing an electric wire carrying a strong current is as great as that to which a person ignorant of the ways of snakes would be subjected if he undertook to take the place of the skilled observer . . . accustomed to put his arm into a tall jar containing rattlesnakes and take them out." This extract will show the general drift of an article on "Dangers from Electricity," by John Trowbridge, which appears in the *Atlantic* for March. There is a paper by Charles Worcester Clark on "Woman Suffrage, Pro and Con;" George Parsons Lathrop shows us "The Value of the Corner;" and there is a paper called "Loitering through the Paris Exposition," which tells, among many other things, of all the concerts given at the cafés of the exposition by the various nationalities, — Gypsies, Javanese, Hungarians, and many more. Dr. Holmes is particularly amusing in "Over the Teacups," and seems to wish that people would write less poetry. He closes with some odd verses on the rage for scribbling.

LETTERS TO THE EDITOR.

Physical Fields.

I THINK Professor Dolbear misunderstands the motive of my communication relative to physical fields, that appeared in *Science* Jan. 24. It was not so much what I conceived to be misuse of the term "stress," that I wished to call attention

to, but rather what I believed to be a misconception of the nature of certain phenomena which such misuse seemed to imply. Let me see if I can maintain my ground.

If two bodies connected by an elastic medium retain their relative positions, the two may be transported cr caused to move in any or all possible ways, and still with all speeds; yet the condition of stress under which this elastic connecting medium exists is not changed at all. If a force be exerted upon one of these bodies, tending to change its position relative to the other, the stress of the elastic connecting medium will be changed; and I do not think it necessary to conceive of a rate of propagation of this modified stress from the one object to the other, for, if the second body were not attached to the first in some way, the force applied to the first could produce no stress whatever in the medium connecting the two. A push on one becomes a pull on the other, but there could be no push on the one without there being an exactly equal and opposite effect upon the other. Has any propagation taken place in this case?

If, again, we have a system of bodies, all of which are connected with each other by elastic strings or by a pervading elastic medium, any movement of one of these bodies necessarily involves a change of stress between all of them. A push on one means a pull of exactly equal amount on others. There can be no push without a resistance, and this resistance is a

Publications received at Editor's Office, Feb. 17-22.

KANSAS Academy of Science, Transactions of the Twentieth and Twenty-first Annual Meetings of the, 1887-88. Vol. XI. Topeka, State. 127 p. 8°.

U. S. COAST AND GEODETIC SURVEY. Chart showing Annual Change of the Magnetic Declination for the Epoch January, 1890. Washington, Government. Scale 1 : 10,000,000.

——Chart showing Magnetic Meridians of the United States for January, 1890. Washington, Government. Scale 1 : 10,000,000.

——Isogonic Chart for the Epoch 1890. Alaska and Adjacent Regions. Washington, Government. Scale 1 : 13,700,000.

——Isogonic Chart of the United States for the Epoch 1890. Washington, Government. Scale 1 : 7,000,000.

WARD, H. M. Diseases of Plants. London, Society for promoting Christian Knowledge; New York, E. & J. B. Young & Co. 196 p. 16°. $1.

WAUTERS, A. J. Stanley's Emin Pasha Expedition. Philadelphia, Lippincott. 378 p. 12°. $2.

WEDDERBURN, A. J. A Popular Treatise on the Extent and Character of Food Adulterations. (U. S. Dept. Agric., Bulletin No. 25.) Washington, Government. 61 p. 8°.

WHIST, American, Illustrated. By G. W. P. Boston and New York, Houghton, Mifflin, & Co. 367 p. 16°. $1.75.

quality of the elastic medium given to it by the mere presence of the other body. We have seen, in the case of the two bodies connected by an elastic string, that the stress is due to the relative positions of the two bodies. They were brought into their position by motion; but, so soon as this motion was overcome by the elastic resistance of the string, the force applied became potential, not kinetic.

Now, supposing one of these bodies to remain stationary while the other is moved farther away, the stress between the two is increased. The push on the one has increased the pull on the other, yet there has been no motion of the latter. The only motion there was in the system was that imparted to the former; and the motion was away from the latter, not towards it. A new static condition has been induced upon both of them, but can it be said to have been propagated from one to the other? I do not think it can.

Now, these two bodies thus connected may be moved from one place to another, yet, so long as they maintain their same relative positions, the stress or tension of the elastic between them will not vary. So, if a form of stress between two bodies in space may be conceived to be entirely independent of the presence of any other bodies in space, a relative motion of these two bodies between themselves involves a change of stress without propagation; and, again, if these two be relatively still, they may move relatively to all other bodies without changing their stress or altering their static condition with regard to stress; yet in one sense (and I conceive in this sense alone) is the stress properly said to be propagated.

Professor Dolbear says, "One may call it potential or kinetic energy if he chooses: a static condition will presently be reached, but not instantly."

Right here, I suppose, lies the gist of the whole thing. The point I wished to make was that Professor Dolbear did not distinguish between the condition of the medium in the two states of motion and rest. A potential condition involves motion only in so far as motion was necessary to bring it into being. It may in one sense be considered as stored-up motion, as it is capable of giving out again an equivalent quantity o motion, but it is not motion itself. He implies, and I agree with him, that motion precedes the potential condition. Now, this motion, or energy if you choose, may be of any known kind (not necessarily electrical, but may be); and when, by reason of the work done, we have produced a condition of matter of a certain kind, — when all the work has been done, — we have a conditon that is called electrification.

What I have been contending for is that the magnetic field and the fields of electrification and gravity are those fields when they are established (before that, in the interval during which they are being established, the fields are kinetic, not potential); that in the case of electrification the movement necessary or involved in its establishment is not the electric field, but is the electric current which can be propagated, and the condition of stress produced by that motion is the electric field.

It is possible, however, that I have not fully understood him. Perhaps he means something like this: imagine, for instance, a row of material points numbered from 1 to 10 spread out in a row, and connected by elastic thread. If 1 and 10 are stationary, and the intermediate numbers are strung along the string, the tension upon the string is uniform throughout its length. If, now, No. 1 be moved a little farther from No. 2 than originally, the tension on the string between the two would be greater than before. This would cause a slight movement of No. 2; and so on to No. 10, which we have imagined stationary. Would it take *time* for the pull on No. 1 to become apparent on No. 10? It would with all material elastics, because of their viscosity and inertia; but, if we use as our elastic the luminiferous ether, I maintain that it would not, for one of the main features assigned to it is absolute and perfect elasticity. If No. 1 were pushed nearer to No 2, the lessening tension would exist at No. 10 at the same instant, because another property of the ether is absolute incompressibility.

Taking this latter characteristic of the ether, — incompressibility, — if we had a long pole (say, a thousand miles long), if we should cause one molecule of that pole to change its position, would not every other molecule, even those a thousand miles away, be caused to move also? and would there be any time lost in their responding? Or, to put it in another way, if we should premise that the farthermost molecule should not move, would it be possible in any way to move the nearer molecules? I contend that it would not, and that if one moved, all must move.

He says, "Mr. Perry seems to say, that, if there was but one body in the universe, it could not have an electric field, even if it could be electrified." That is not my statement, nor is it my idea. My idea is, that in the case of static fields, under which head I would include electrification, magnetism, and gravity, two exactly equal and opposite conditions are inevitable. I cannot conceive of there being a push without there being a corresponding and equivalent pull, without the destruction of equilibrium; and, if the equilibrium be destroyed, we have motion which may be in any direction whatever. This is what I conceive to be the difference between the two kinds of fields. As before stated, I do not believe a pull can exist without a corresponding push and yet maintain equilibrium. If the one exceeds the other, there will be motion towards the greater, — there will be more positive than negative electricity; there will be more north-seeking magnetism than south-seeking magnetism, or *vice versa;* and by the same token one may exist without the other. By the same course of reasoning, if a stress can be propagated (as I use the term "stress"), there is implied an existence of one form (the positive) before the existence of the other (the negative), as the element of time is involved. Faraday distinctly states that there cannot be an absolute charge of matter. I stated my belief that a stress could not exist unless there were two particles. I should also have added that there must be a connecting medium between those two particles. If this be so, then it is clear without reasoning, that, if either one of the particles or the connecting medium be wanting, the conditions for stress are wanting, and it cannot exist. A material body having two points and a connecting medium between the two is therefore capable of either magnetism or electrification. If one of the points be wanting, and energy be applied to the other, this energy, instead of being stored up by the tension of the elastic medium, and producing stress as before, — which would be capable of giving out again an equivalent amount of energy or motion, which stress might constitute electrification or magnetism,—produces no stress, but motion at once, which may be heat, light, or the electric current, or give rise to these.

Again: if there were but a single body in space, its physical field would, I think, be confined within itself, and not radiate outward indefinitely. Let us imagine space to consist of an elastic jelly: then all particles of matter in space are connected with each other by elastic bonds. One particle cannot be moved from its position without setting up stresses in space between itself and all the other particles. But it is evident that the algebraic sum of all the stresses is zero. If the stress be wholly positive on one body or particle, the stresses on all the other particles will be negative and exactly equal in amount, whether there be a million or only one other particle. If there be but one other particle or body, all of the negative stress — electricity, if you choose — will be upon it, or perhaps rather in the medium joining the two. Now, since the stress lies wholly between these two (they are in no way connected with any other particles, or, in other words, they are the only two particles in space), they may be moved in any way, providing their positions relatively to each other remain the same, without altering the stress of the surrounding medium. Since they do move, there is kinetic energy; but this movement does not alter their relations to other particles, because there are no other particles: hence no additional stresses are set up. Their movement does not convert the potential energy stored up between them into kinetic energy, although a movement of one relative to the other would do so; and the resulting kinetic

energy would represent the difference between the initial and final potential states of the system.

He quotes Tait as saying, "Every action between two bodies is a stress," and says that "the body and the ether about it are two bodies, and, if they can act at all upon each other, there will then be a field." But you will recollect that he makes this statement in controversion of mine, that, if there were but "a single mathematical point in space, there could be no stress." I said nothing at all about there being such a thing as ether in this connection, though I see the necessity of including it, and also the force of his argument; but I do not think Tait meant to consider the ether as a body in the sense in which Professor Dolbear here uses it. As I understand it, Faraday, Maxwell, Thomson, and I assume Tait also, believe the dielectric to be the active agent, and the conductor the passive agent, in all the phenomena which we are considering. The dielectric, whatever it may be, — the ether, if you will, — is really the seat of the strains which terminate in the two bodies connected. I think Tait used the term in the sense that I have indicated.

"Perhaps, however, Mr. Perry calls the ether matter, which has not been my habit, and against which I was not on my guard when I wrote the statement to which he objects. Until we have some evidence that ether is subject to the law of gravitation, it seems to me to be improper to speak of it as matter. If every particle of matter attracts every other particle of matter, and if there is no evidence that ether is so attracted, it is not conducive to good terminology to call it matter."

Let us see what authority we have for considering ether as matter. I believe the weight of opinion is either that the ether is a form of matter or that matter is a form of ether. Sir William Thomson believes that matter is nothing but ether; that it is composed of it. We know this all-pervading medium as ether when it is unorganized. When it is organized into vortex rings, we have the atom and molecule, hence gross matter, as it is usually distinguished. I am of the opinion that Sir William Thomson's theory of matter is the most popular one at present. In 1838 M. Pouillet found that the heat-energy transmitted from the sun to the earth would, if none were absorbed by our atmosphere, raise 1.76 grams of water 1° C. in 1 minute on each square centimetre of the earth normally exposed to the rays of the sun. This is equivalent to 83.5 foot-pounds of energy per second. This figure Sir William Thomson used in determining the probable density of the ether.

Herschel estimated the stress (elasticity?) of the ether at 17×10^9 pounds per square inch. S. Tolver Preston estimates the probable inferior limit of the tension of the ether at 500 tons per square inch, which is much smaller than Herschel's estimate. Young remarks, "The luminiferous ether pervading all space is not only highly elastic, but absolutely solid." I do not understand the meaning attaching to "solid" here, but it is evidently an attribute of matter. Sir William Thomson, calculating upon the data above referred to, finds the weight of a cubic foot of ether to be $\frac{4}{5} \times 10^{-20}$ pounds. Bellini makes it $\frac{1}{2} \times 10^{-13}$ pounds. M. Herwitz, another investigator, arbitrarily assumes a cubic foot of ether to weigh 10^{-18} pounds.

De Volson Wood treats the ether as if it conformed to the kinetic theory of gases, which, with other assumptions, is equivalent to considering it as gaseous in its nature, and at once compels him to consider it as molecular. He says, "The electro-magnetic theory of light suggested by Maxwell (?), as well as the views of Newton, Thomson, Herschel, Preston, and others, are all in keeping with the molecular hypothesis."

Professor Rood succeeded in producing a vacuum of $\frac{1}{390,000,000}$ of an atmosphere. Professor De Volson Wood states, that, even at this great rarity of the atmosphere, the quantity of matter in a cubic foot of air "would be some 200 million million times the quantity in a cubic foot of ether," and says, that, admitting that the ether is subject to attraction according to the Newtonian law and of compression according to the law of Mariotte, in order to make the density vary sensibly with the distance, the attraction of the central body must be something like a million times as great as that of the sun, or have a diameter a million times as large; but, there being no such known body, he concludes that the density and tension of the ether may be considered uniform throughout space; and he says that the weight of a given volume of it would vary as the force of gravity, and places the weight of a cubic foot of ether at the surface of the sun at 57×10^{-24} pounds, and estimates the pressure on a square foot of the sun of a column of infinite height at 13×10^{-14} pounds.

Thus we see, that, while no two of these investigators agree in their results, they all agree in ascribing to the ether all the properties of matter, including that of gravity, and I therefore think it no violation of the proprieties to speak of it as though it were matter.

In regard to the definition of the word "stress," Professor Dolbear quotes Maxwell as follows: "Now, we are unable to conceive of propagation in time except either as the flight of a material substance through space or the propagation of a condition of motion or stress in a medium already existing in space," and says, "Evidently Maxwell did conceive that stress could travel." I freely admit that a "condition of stress" may travel, in the sense that a body between the particles of which there exists a stress may travel; and it seems to me that is what Maxwell means. If he meant what Professor Dolbear thinks he does, why does he says a "condition of stress"? Why not simply "stress"?

I think Maxwell was probably the first to use the term "stress," but it was in relation to phenomena described by Faraday. In regard to this, Maxwell himself says (vol. i. p. 153), "The distribution of stress considered in this chapter is precisely that to which Faraday was led in his investigation of induction through dielectrics." Further, he says, "This is an exact account of the conclusions to which we have been conducted by our mathematical investigation. At every point of the medium there is a state of stress such that there is tension along the lines of force, and pressure in all directions at right angles to these lines." "The expression 'electric tension' has been used in various senses by different writers. I shall always use it to denote the tension along the lines of force, which, as we have seen, varies from point to point, and is always proportional to the square of the resultant force at the point." "The hypothesis that a state of stress of this kind exists in a fluid dielectric," etc. "The state of stress which we have been studying." "If the medium is not a perfect insulator, the state of constraint which we call electric polarization is continually giving away. The medium yields to the electro-motive force, the electric stress is relaxed, and the potential energy of the state of constraint is converted into heat." "In the phenomenon called the electric current, the constant passage of electricity through the medium tends to restore the state of polarization as fast as the conductivity of the medium allows it to decay. Thus the external agency which maintains the current is always doing work in restoring the polarization of the medium which is continually becoming relaxed, and the potential energy of this polarization is continually being transferred into heat."

I consider the above as perfectly in accord with my statements in your issue of Jan 24.

On p. 257, § 642, he specifically defines "stress" as follows: "Hence the state of stress may be considered as compounded of (1) a pressure equal in all directions, (2) a tension along the line bisecting the angle between the directions of the magnetic force and the magnetic induction, (3) a couple tending to turn every element of the substance," etc. "The stress in this case is therefore a hydrostatic pressure, combined with a longitudinal tension along the lines of force," etc.

But Faraday was the first to conceive of these stresses, although I am not sure that he used this term. In his "Experimental Researches," 3249, he says, "With the electric force we have both the static and dynamic state; . . . still there are well-established electric conditions and effects which the words 'static,' 'dynamic,' and 'current' are generally

employed to express. . . . The lines of force of the static condition of electricity are present in all cases of induction. . . . No condition of quality or polarity has as yet been discovered in the line of static electric force, nor has any relation of time been established in respect of it." "No relation of time to the lines of magnetic force has as yet been discovered" (*Ibid.*, 3253).

Finally, on pp. 439 and 440 of "Experimental Researches" (vol. iii. edition of 1855), he gives in detail, too long for quotation here, his views of the different phenomena, which, it seems to me, fully support the position I have taken in this matter.

NELSON W. PERRY.

Cincinnati, O., Feb. 17.

Supposed Aboriginal Fish-Weirs in Naaman's Creek, near Claymont, Del.

IF the substituted letter of Mr. Hilborne T. Cresson to the *American Antiquarian*, published in your issue of Feb. 14, had ever been printed before, certainly I should not have received the impression that Mr. Cresson once fancied he had discovered the remains of pile-dwellings at Naaman's Creek, on the Delaware. The differences between the two versions are very striking to whoever takes the trouble of comparing them. I never before understood that Mr. Cresson regarded the version of his letter published in the *Antiquarian* in November, 1887, as "an atrociously garbled version" of it. I supposed he only complained of certain bad mistakes in the proof-reading, such as the substitution of "cave" for "cove," etc. Mr. Cresson's memory has played him false in regard to what he wrote to me when he kindly forwarded to me a selection of the objects discovered at the three "stations." On referring to the notes that accompanied the specimens, I find that he calls them "pile-structures." The fact is, that I supposed Mr. Cresson had changed his mind in regard to what these structures actually were; and as I had formed the opinion upon first reading what he had printed respecting them, that they were merely remains of Indian fish-weirs, I simply made that statement. I found nothing in what Professor Putnam had stated in the "Reports of the Peabody Museum" (vol. iv. p. 44) in regard to Mr. Cresson's discoveries to give me any different impression. Mr. Cresson's letter to me, to which he refers, containing the request that I should adopt his corrected views, came too late, as I wrote to him, because my manuscript was already in the printer's hands. That I should have drawn such inferences about Mr. Cresson's opinions does not seem to me so "inexplicable" as it does to him.

HENRY W. HAYNES.

Boston, Feb. 16.

MR. H. T. CRESSON, in his letter published in *Science*, Feb. 14, seems to want to get away from his own assertion, and so takes the opportunity to abuse the editor of the *American Antiquarian*. If you will allow me to quote the very words which he used in his letter, and which were published in the *Antiquarian* exactly as they were written, without any change whatever, your readers will see what his position was in the year 1887, though he seems to have changed his opinion since that time. The words are as follows:—

"The results so far seem to indicate that the ends of the piles embedded in the mud, judging from the implements and other *débris* scattered around them, once supported *shelters of early man that were erected a few feet above the water*—the upper portions of the *piles* having disappeared in the long lapse of time that must have ensued since they were placed there — (the flats are covered by four and one-half feet of water on the flood tide; on the ebb the marsh is dry and covered with slimy ooze several feet in depth, varying in different places). Three different *dwellings* have been located, all that exist in the flats referred to after a careful examination within the last four years of nearly every inch of ground carefully laid off and examined in sections.

"The implements found in *two* of 'the supposed *river dwelling sites*,' are very rude in type, and generally made of dense argillite, not unlike the palæoliths found by my friend Dr. C. C. Abbott in the Trenton gravels.

"The character of the implements from the other or third supposed *river dwelling* on the Delaware marshes are better finished objects made of argillite, indicating a greater antiquity than ordinary surface found Indian relics. At this *pile dwelling* a human tooth has been found and fragments of a jaw bone, ends of scapulæ, etc. It is my intention later on to present my specimens to the Peabody Museum of Ethnology and Archæology at Cambridge, Mass."

The above is a quotation from the letter published in the *American Antiquarian* in 1887. Mr. Cresson desires the readers of *Science* to compare the two letters. In order that they may do so, I quote a part of the letter which appeared in *Science*, Feb. 14 (see p. 116, near the bottom of the page). It is as follows:—

"The results, so far (1877), seem to indicate that the ends of piles embedded in the mud, judging from the implements and other *débris* scattered around them, had once served as supports to structures intended for *fish-weirs*, these in all probability projecting a few feet above the water, and were no doubt interlaced with wattles, or vines, to more readily bar the passage of fish from the creek into the river. The upper portion of these *wooden structures* has entirely disappeared in the long lapse of time that has ensued since they were placed there. . . . At slack water it forms a low mud-bank slanting toward the creek. Three different *stations* were located, probably all that exist, in the bed of the creek referred to. This opinion is based upon careful examinations, made within the past four years, of nearly every inch of ground in the neighborhood of the wooden stake-ends, by dredging in sections between certain points marked upon the creek's bank. The implements found in one of the *stations* are generally made of argillite, with a few of quartz and quartzite. Some were very rude in character, and not unlike the palæoliths found by Dr. C. C. Abbott in the Trenton gravels. Objects of stone and pottery rather better in finish than those at *station* A have been found at the two other *stations*, B and C."

This is a quotation from *Science*, the sentences being consecutive. The Italics will show the words and clauses which in one letter convey one impression, and in the other letter convey an entirely different impression.

Mr. Cresson charges the editor with putting in the words "shelters of early man that were erected a few feet above the water," "three different dwellings," "two of the supposed river dwelling sites," "The character of the implements from the other or third supposed river dwelling on the Delaware marshes are better finished objects made of argillite, *indicating a greater antiquity* than ordinary surface found Indian relics. At this *pile dwelling* a human tooth has been found and fragments of a jaw bone, ends of scapulæ, etc." Now, the editor of the *American Antiquarian* does not pretend to be ingenious enough to fabricate such sentences, and interpolate them into a letter. It is beyond the skill of an ordinary man to interpolate remarks of that kind. If these words are not contained in the copy which Mr. Cresson says he kept, why did not Mr. Cresson change the wording, or request that it should be corrected, in the two years that have elapsed? Professor Haynes quoted from the *American Antiquarian*, supposing that Mr. Cresson's own words were to be relied upon. The statement went into "The Critical and Narrative History" on the strength of Mr. Cresson's own words. The editor of the *Antiquarian* at the time said nothing about the "find." If Mr. Cresson wishes to withdraw from the position taken, he is at liberty to do so, but he should not charge the editor of the *Antiquarian* with "garbling" or changing his letter, unless he can prove it.

STEPHEN D. PEET.

Mendon, Ill., Feb. 18.

The Fiske Range-Finder.

I WAS much interested in the description of the Fiske range-finder, which appeared in *Science* on Jan. 24. There is much credit due Lieut. Bradley A. Fiske for the ingenious manner in which he has applied a most beautiful electrical combination to a practical purpose, and there is no doubt that its range of usefulness will extend beyond the realms of gunnery practice.

While reading the article, an idea came into my mind, which may also have occurred to Lieut. Fiske, and been rejected as im-